

# NIH Needs a Deep Innovation Program

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# The Issue:

**The Next “Little Thing”**

**vs.**

**The Next “Big Thing”**

# Opportunities for Deep Innovation:

- *Move to advocacy vs. fault-finding*
- *Reduce our over-reviewing*
- *Welcome the allied disciplines*

# Problems for Innovation

- Communal review is conservative
- Some reviewers are competitors

# Predictability & Detail

- Proposals are too long
- Too reliant on preliminary data
- 3-digit scoring is not meaningful
- We can't see the unforeseeable

# We Should Harness the Allied Disciplines

Physics, Chemistry, Math, Computer  
Science, Engineering

# Biology's Windfalls

X-ray Crystallography, Synchrotrons, NMR,  
Electron Tomography, AFM, Molecular  
Tweezers, CT Scans, Computational  
Biology, Mass Spectrometry, DNA  
Sequencing, ...

# Protein X-Ray Crystallography

- 1895 Roentgen X-rays
- 1912 von Laue, Braggs Diffraction
- 1955 Perutz, Kendrew Globin Structures

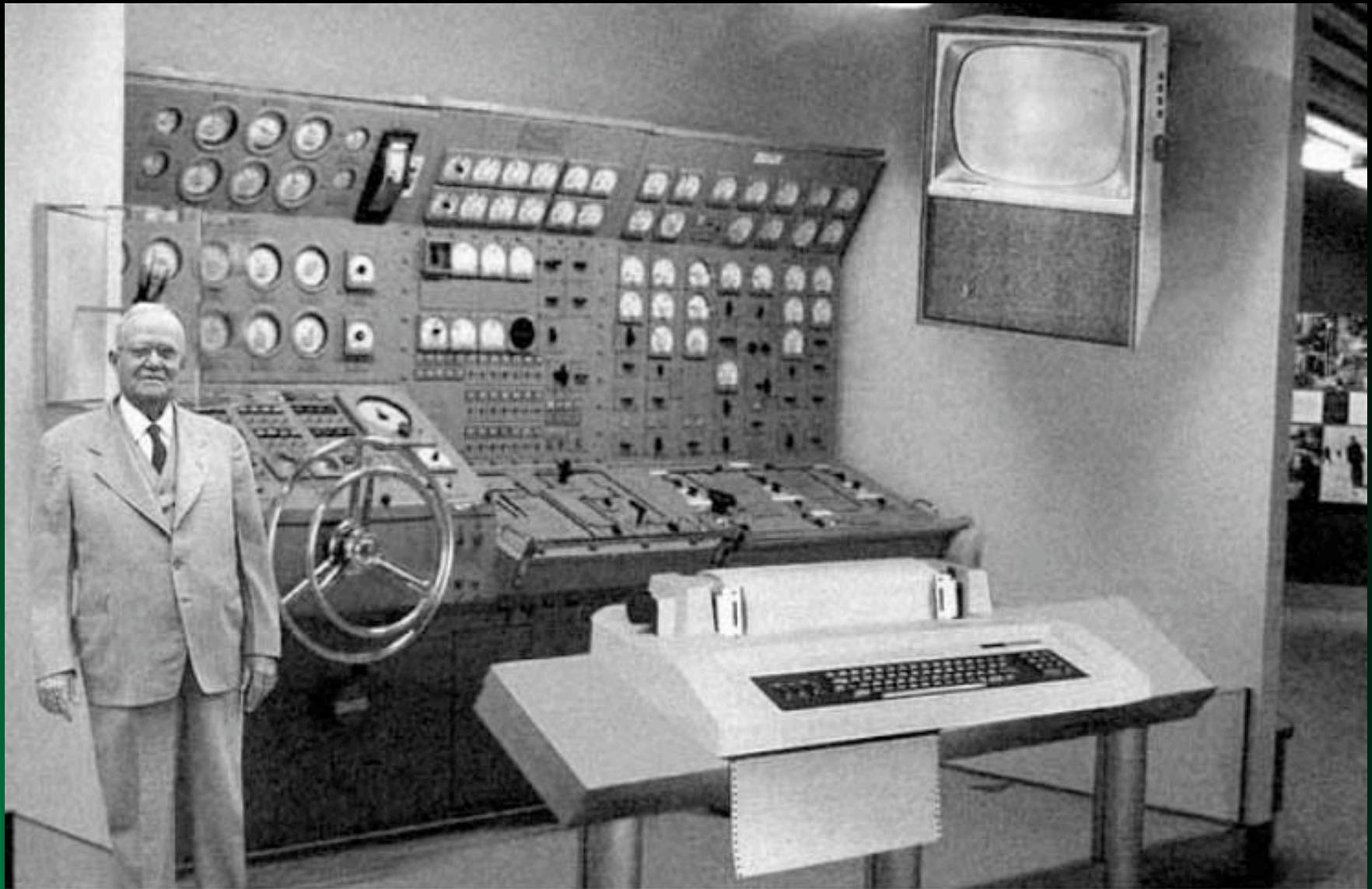
# NMR in Biology

- 1945 Bloch, Purcell      Nuclear Magnetism
- 1966 Ernst              Pulsed Mag Fields
- 1982 Wuthrich          Protein Structures

# Physical-Science Payoffs for Biology

- **Broad:** Impacts Much of Molecular Science
- **Transformative:** More than 20 Nobel Prizes
- **Slow:** Often 10 - 50 Years
- **Unforeseeable:** Biological Relevance

We're Not Good at  
Predicting Big Advances



*Scientists from the RAND Corporation have created this model to illustrate how a "home computer" could look like in the year 2004. However the needed technology will not be economically feasible for the average home. Also the scientists readily admit that the computer will require not yet invented technology to actually work, but 50 years from now scientific progress is expected to solve these problems. With teletype interface and the Fortran language, the computer will be easy to use.*

“You Rarely find the Most  
Important Things by  
Deliberately Looking for  
Them.”

J Lederberg

We Should Put Less  
Emphasis on  
Guessing the Payoffs

# Our Evaluations Should Separate:

- PI, interesting science
- Predicted payoff, our estimates of relevancy to the NIH mission, and immediacy of impact

# A Proposal for Deep Innovation Grants

- Short: 5-8 pages
- Independent, Arms-length reviews
- Focus on people, not payoff
- Relevance separated from review
- Ranking, not Scoring

# Opportunities for Deep Innovation

- Let our best people explore: Perutzes, Pat Browns, ...
- Develop methods, principles, underpinnings, new ideas
- Attract new scientists to biomedical research

# Ranking vs. Scoring

- No need for reviewers to meet
- Better leverage of reviewers' insights
- Robust against blackballing
- Allows for advocacy
- It respects our reviewers

Grant Number	Ranking
2	1
4	2
6	3
8	4
7	5
1	6
3	7
5	8

We have 4 reviewers: A, B, C, and D. Each Reviewer gives 3 0's (unranked) and scores 1, 2 and 4 from worst to best: 6 total reviews. The matrix below shows a possible scoring from the 4 reviewers.

Grants	Reviewers				Total
	A	B	C	D	
1	0	-	0	0	0
2	4	-	4	4	12
3	0	0	-	0	0
4	2	4	-	1	7
5	0	1	0	-	1
6	1	2	2	-	5
7	-	0	1	0	4
8	-	0	0	2	2

# Explorers, Pioneers & R21's

- Not “High-Risk High-Reward”
- We need 100's, not 10's
- We need grants, not awards
- Should be untargetted
- One size does not fit all
- We need a gateway to other fields

# Deepening Biomedical Research

- It falls to NIH
- NSF, DOE, DARPA won't
- Google's Rule: 70/20/10